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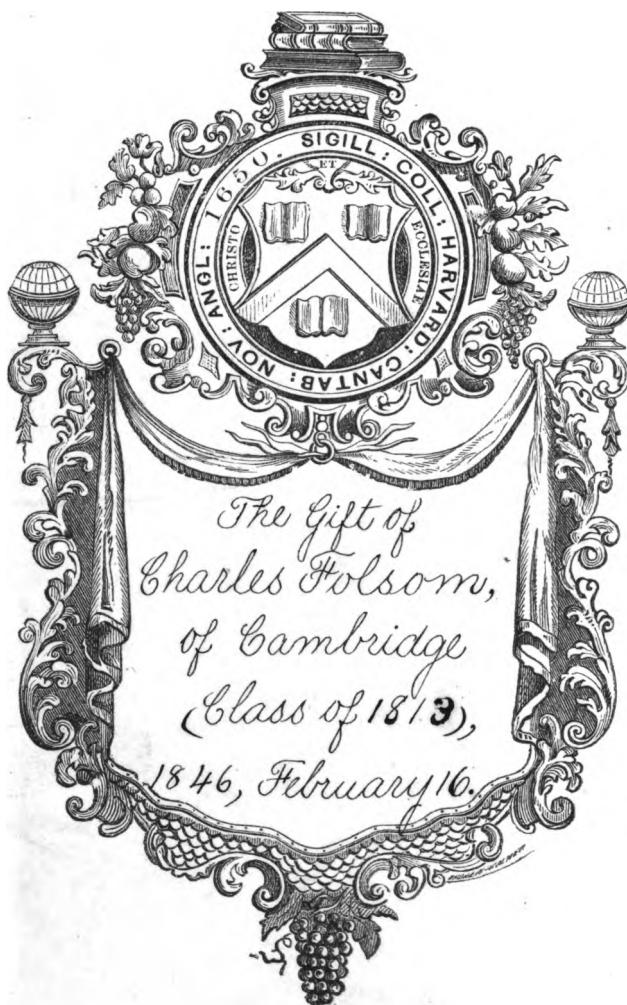
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NEWTON AND FLAMSTEED.

REMARKS

ON

AN ARTICLE IN NUMBER CIX.

OF THE

QUARTERLY REVIEW.

SECOND EDITION.

TO WHICH ARE ADDED

TWO LETTERS,

OCCASIONED BY A NOTE IN NUMBER CX. OF THE REVIEW.

BY THE

REV. WILLIAM WHEWELL, M.A.

FELLOW AND TUTOR OF TRINITY COLLEGE, CAMBRIDGE.

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R E M A R K S,

&c.

IT is, for the most part, a sorry occupation to rake up the quarrels and infirmities of persons who have long been the objects of gratitude and respect among mankind. Yet there are cases in which this, though a painful, may be a proper employment. When we have, brought before us, a vast quantity of new and curious documents and evidence, such as has recently been furnished in Mr Baily's singularly valuable and interesting "Account of Flamsteed," it becomes the business of those who care for the good name of great men to revise their opinions of the persons whose characters may be affected by the disclosures contained in such a work. It was therefore very reasonable that those who undertake the office of directing the judgment of the public, as far as it is concerned about literary novelties, should notice any new light which was thrown upon the well known quarrel of Halley and Flamsteed by Mr Baily's excellent publication.

But the Quarterly Reviewer who has taken upon himself this task, appears to me to have performed it without due attention to the most important circumstances of the case. In a dispute where it is very clear that there was great irrita-

A

tion and anger on both sides, he has taken for his sole guide the statements of one of the parties, written in the warmth of the moment, has identified himself with Flamsteed's most petulant feelings, and has not corrected them by any attention to the case of the opposite party. When the great body of Review Readers are called upon, in this temper, to cast away all their reverence for the most revered name of our nation, it must be right that some one should interpose a warning, and deprecate judgments of such levity and partiality. I would very gladly have left this task to some lover of truth who could give to it more time and labour; for the whole evidence is of very great extent; but, no such person appearing to come forwards, I will at least make a few remarks which may serve to arrest such a sentence as is moved for by the Reviewer.

It is to be observed that if we adopt the Reviewer's opinion, that Flamsteed was throughout a man bitterly wronged, and that there was an extreme of baseness and tyranny on the side of the persons with whom he quarrelled, we involve in our condemnation almost all the eminent literary and scientific men of the day: for we have, acting with Newton, and sharing in his views, not only Halley, the object of Flamsteed's intense dislike, but Gregory, Arbuthnot, Mead, Sloane, Wren. On the other hand we find no one speaking of Newton as Flamsteed does, except Whiston, whose judgment is perfectly worthless, for he was a prejudiced, passionate, inaccurate and shallow man, as might easily be shewn, and as is, I think, commonly allowed. Such a comparison of parties may at least make us pause.

One of the most important remarks bearing upon this unfortunate quarrel is, in my opinion, this: that, the subject of dispute being the publication of Flamsteed's observations and their results, Newton and Flamsteed, from the direction of their pursuits, took views so opposite, of the best manner and proper purpose of such a publication, that they unavoidably differed in their wishes, and might disagree with little or no blame on either side. The purpose for which Newton desired that the world should possess the best observations, was the confirmation of the great Theory of Universal Gravitation;—incomparably the greatest discovery ever made by man; and at that period, we may say, in the agony of that latent struggle by which the confirmation and general reception of great discoveries is always accompanied. We of the present day are accustomed to consider this immense step as effected at once, on the publication of the first edition of the Principia in 1687; but we may easily convince ourselves that this was not so. Even under the most favourable circumstances, a vast theory like this could not make its way at once. No man of Newton's standing (I believe) thoroughly accepted his views: Halley was sixteen, David Gregory nineteen years his junior. In England this acceptance of the theory required half a generation, in France and Germany, more than a whole generation. And during this interval, the result of the struggle depended upon the accordance of the theory with the best observations, which the Greenwich ones undoubtedly were. Upon these observations, then, depended a greater stake in the fortune of science than was ever before at hazard, and this Newton

knew well. How then can one be surprized at the earnestness and importunity with which he begs for Flamsteed's observations; and tries to soothe a jealousy and reserve which appear to have shewn themselves at an early period?

As for your observations, you know I cannot communicate them to any body, and much less publish them, without your consent. But if I should perfect the moon's theory, and you should think fit to give me leave to publish your observations with it, you may rest assured that I should make a faithful and honorable acknowledgment of their author, with a just character of their exactness above any others yet extant. In the former edition of my book, you may remember that you communicated some things to me, and I hope the acknowledgments I made of your communications were to your satisfaction: and you may be assured I shall not be less just to you for the future. For all the world knows that I make no observations myself, and therefore I must of necessity acknowledge their author: and if I do not make a handsome acknowledgment, they will reckon me an ungrateful clown.—

Account of FLAMSTEED, p. 151.

This the Reviewer has quoted; but he has not quoted what immediately follows, striking as it is.

And, for my part, I am of opinion that for your observations to come abroad thus with a theory which you ushered into the world, and which by their means has been made exact, would be much more* for their advantage and your reputation, than to keep them private till you die or publish them, without such a theory to recommend them. For such theory will be a demonstration of their exactness, and make you readily acknowledged the exactest observer that has hitherto appeared in the world. But if you publish them without such a theory to recommend them, they will only be thrown into the heap of the observations of former astronomers, till *somebody shall arise* that, by perfecting the theory of the moon, shall discover your observations to be exacter than the rest. But when that shall be, God knows: I fear not in your life-

* Erroneously printed "worse" in the work.

Theory
confirms
the data?

time, if I should die before it is done. *For I find this theory so very intricate, and the theory of gravity so necessary to it, that I am satisfied it will NEVER be perfected but by SOMEBODY WHO UNDERSTANDS THE THEORY OF GRAVITY AS WELL OR BETTER THAN I DO.*—p. 151-152.

I have several times, in reading this passage, felt a kind of terror at the peril to which the success, or at least the speedy success, of the greatest of physical truths is here represented as exposed.

With this consciousness of being in possession of such a truth, while Flamsteed's records of his observations contained the only language in which it could be made generally convincing, we may easily imagine that Newton could not help urging the publication and employment of the observations, in a manner which excited no sympathy in Flamsteed, unconscious of the nature of the then existing crisis in the history of astronomy.

Flamsteed was only four years younger than Newton; he never fully accepted Newton's theory, nor comprehended its nature. Like all astronomers of his time, he understood by "theory" only a mode of expressing *laws of phenomena*, not a new generalisation by which such laws are referred to a physical *cause*. When he talks of his own Theories of the Moon and Planets, it is in such a sense. The truth of what is here asserted is evident from many passages of the book now under consideration, but especially from a letter of Flamsteed's to his friend Lowthorp, dated May 10, 1700. When he was told that Newton had deduced all the inequalities of the moon's motion from the laws of gravity alone,

With some indignation I answered that he had been as many years upon this thing, as I had been on the constella-

tions and planets altogether: that he had made lunar tables once to answer his conceived laws, but when he came to compare them with the heavens, (that is, the moon's observed places,) he found he had mistook, and was forced to throw them all aside: that I had imparted above 200 of her observed places to him, which one would think should be sufficient to limit any theory by; and since he has altered and suited his theory till it fitted these observations, 'tis no wonder that it represents them: but still he is more beholden to them for it than he is to his speculations about gravity, which had misled him. Mr Hobbs boasted that his laws were agreeable to those of Moses. Dr Eachards tells him he doubted not of it, for being drawn from Moses' works, and copied into his, he might be sure they would agree, except the laws of Moses were flown, which he was sure they were not.—p. 176.

It is manifest here that Flamsteed attached no more value to Newton's laws of nature than he did to Hobbes' laws of nations.

Flamsteed's view of the value of observations and of their publication was probably nearly the same as that of the mere practical astronomers of all ages; that is, that the observations were to be compared with known laws, so as to improve their general accuracy; and that then, if the occasion occurred, additional laws of phenomena should be made out by conjectures empirically confirmed. But besides this view, in which there is nothing to blame but its limited character, he appears to have thought too directly of their value as the means of purchasing reputation. How otherwise are we to account for the jealousy with which he objected to Newton's combining Cassini's observations of the comet of 1680 with his? when it must have been clear, even with his own notion of a theory, that the truth of the theory would be the better established, the more observations it agreed with. This is Flamsteed's

own account of an interview with Newton in the letter just quoted:

Some occasion of discourse about comets happening, I acquainted him that Dr Gregory gave out that since he had altered his paths of comets, and instead of parabolas made them ellipses, his theories would represent all Mons. Cassini's observations within a minute, whereas I thought he had only *my* observed places to represent, and that it was not only an injury *to me*, but the nation, to *rob* our Observatory of what was due to it, and further to bestow it *on the French*.—p. 174.

With these feelings, we can easily imagine that Flamsteed might wish to secure what he conceived due to the character of himself and the nation (for certainly the latter had a large share in his thoughts) in the splendour, correctness, and completeness with which his results were published, rather than to attend to the needs of astronomy, which so much called for expedition. And such appears to have been the ground of the unhappy disputes between him and his opponents.

This fundamental difference of object between Flamsteed and Newton would of itself make it necessary to receive with reserve and mistrust the judgments of the former when the difference had grown into a quarrel. But we cannot but perceive, I think, independently of this, that Flamsteed was not a man on whose judgment or fairness we can rely. A good and conscientious man he certainly was; but, of weak health from childhood, he seems also to have been of weak temper, suspicious, irritable and self-tormenting. We can hardly think otherwise of a man who was in the habit of brooding over the movements of spleen excited by casual expressions in the letters of his correspondents, and

recording them in ink on the paper. Thus, as early as 1694, Newton happens to write to him (p. 139) "I believe you have a wrong notion of my method in determining the moon's motions"; on which Flamsteed makes this note, "*I had: and he of me: and still has.*" And after this period almost every letter of Newton's has a similar comment appended to it, and these become more and more bitter. Yet Newton appears to have been his friend as long as Flamsteed's temper allowed him to be so. A little after the above letter he writes:

What you say about my having a mean opinion of you is a great mistake. I have defended you where there has been occasion, but never gave way to any insinuations against you. And what I wrote to you, proceeded only from hence, that *you seemed to suspect me of an ungrateful reservedness, which made me begin to be uneasy.*—p. 146.

Surely there is no want of kindly feeling in these expressions.

The ground of the growing estrangement between them was evidently Newton's intercourse with Halley; who, whatever his demerits might be, had certainly no small share, both in the original publication of Newton's great work, and in the subsequent labours which brought it into its proper place in public estimation. It is highly probable that Flamsteed, a pious and serious man, was disgusted with what he heard, truly or not, respecting Halley's profanity and infidelity. I have fortunately, no occasion to speak of Halley's errors, but I may observe that Flamsteed appears to have lived among men who bandied about such accusations very freely; for on one occasion (p. 229) when he was charged with calling Newton himself an atheist, his remark is,

"I never did: but *I know what other people have said* of a paragraph in his Optics; which probably occasioned this suggestion." Whatever the cause might be, Flamsteed at an early period conceived an extreme dislike to and ill opinion of Halley, and Newton endeavoured in vain to reconcile them. (p. 148.) This repugnance appears in 1692 in a manner which illustrates what I have said, that Flamsteed was likely to quarrel with any one who, from a consideration of the interests of astronomy, urged the speedy printing of his observations.

It only remains that I give you the answer I would make to our suggesting *friend*, when he asks me why I do not print my observations? 'Tis first I do not find myself under any obligations to receive instructions what to do, or be governed by him and his associates, the *Muss's*. [Probably a cant term for those who use the word *Must*.] Secondly, I would not thrust such an incomplete catalogue on the world as he has done from St. Helena: nor be obliged to compliment the best reputed astronomers of our time (as he has done all of them) by telling them that, had their catalogues been extant, he would have called his a supplement to theirs, as he has done (for want of them) of Tycho's.—p. 132.

Halley does not appear to have been wanting in attempts to conciliate his adversary. In 1711 he has occasion to write to Flamsteed, and he ends his letter thus :

Pray govern your passion, and when you have seen and considered what I have done for you, you may perhaps think I deserve at your hands a much better treatment than you for a long time have been pleased to bestow on your quondam friend, and not yet profligate enemy (as you call me), Edm. Halley.—p. 293.

The wrathful temper of Flamsteed's dealings with Newton and his friends, is indeed so manifest, that it is quite marvellous the necessity of making

allowance for it should not have occurred to the Reviewer. Who, for example, can overlook it in the account which Flamsteed has given in his own Diary of his appearance before the Committee of the Royal Society on Oct. 19, 1711, and which the Reviewer has quoted at length? This Committee, it is to be observed, were the guardians of the national interest in the Greenwich observations, and were bound to see that Flamsteed made them accessible and useful to the public. According to his own account he began by calling them "*the robbers of his property.*" In describing the altercation which ensued, he says, "I *only* desired him (Newton) to keep his temper, restrain his passion, and thanked him as often as he gave me ill names." And again, in another part of the conversation: "I *only* desired him (as I had often done) to restrain his passion, keep his temper, &c." We hardly require the recollection of Sir Anthony Absolute to see here the demeanour of a very angry man; far too angry, certainly, to allow us to accept literally what he asserts, much less what he implies merely. I confess therefore I have great doubts whether, from the expression in the same account, "he called me many hard names, *puppy* was the most innocent of them;" (p. 228.) we can confidently infer that the obnoxious term was used.

That Newton's behaviour on this and other occasions betrayed no marks of irritation, I am not at all disposed to believe. The mildness of Newton's character shewed itself rather in his horror of disputes, than in his skill in conducting them. Every one recollects that at an early period of his career he was almost led to abandon even his darling,

Philosophy, when he found that she was such a "litigious lady" as to threaten him with a controversy. In such cases, his impatience of the quarrel was probably not very easily distinguishable from impatience towards the quarreller. And this was still more natural, when he was acting as the authoritative head of a national body, and had, in that capacity, to repress, what must have appeared to him, extravagant claims and offensive behaviour. But surely we are not, at this day, to weigh all the hasty words uttered by Newton or by Flamsteed, any further than is necessary to make us careful of the government of our own tongues and pens.

There is one passage in a letter of Newton's, quoted in the Review, which has, I have no doubt, surprised and offended many readers. I mean that in which, in 1699, Newton expresses displeasure with Flamsteed for having mentioned, in a paper which was about to be printed, that he, Newton, was employed in completing the theory of the moon. When he says, (p. 166), "I do not love to be printed on every occasion, much less to be dunned and teased by foreigners about mathematical things," we can easily refer his expressions to the shyness and love of quiet which we know to have been in his character. But when he adds, "or to be thought by our own people to be *trifling* away my time about them, when I should be about the King's business," we start, as if Newton had uttered treason against the majesty of Science by depreciating her occupations in comparison with "the King's business." A moment's reflexion however, will, I think, satisfy us that there is here nothing to shock us. That Newton at this period thought lightly of

Science and her service, no one can imagine who knows (as any one may know from the documents now published) how carefully he was labouring to complete his gigantic undertaking, and how great were the improvements in the edition of the *Principia* of 1712, over the first edition. Still less can we imagine this, when we turn to the noble views of the nature and objects of philosophy which animate the pages of the "Opticks." But we must not judge Newton as if he were the Hero of a philosophical romance, or a scientific mythology. He was a man of business as well as a mathematician. It *was* his duty to attend to the King's business; to make "Assays of Coins" and to publish "Tables" of them. Whether the men of business and of office with whom he was connected by his situation looked with a doubting eye upon a Warden of the Mint who was the greatest mathematician and philosopher in Europe, I cannot tell: but certainly such opinions, in such persons, on such subjects, would not be without parallel. And if "our own people" had given any indications of a disposition to think that he was trifling away his time, in founding an intellectual empire, instead of attending to the King's business, both prudence and duty required him to guard against inflaming such suspicions. The coldness and severity of Newton's rebuke in this letter are certainly unpleasing; but at least they shew that he was seriously vexed upon the occasion. On the other hand, we can read without any heavy disapprobation Flamsteed's remark on *this* passage, for it breathes his love of his science, although the spleenetic comparison might have been spared. "Was Mr Newton a *trifler*, when he read mathematics for

a salary at Cambridge? Surely astronomy is of some good use, though his place be more beneficial." When it is so obvious that the feeling is substantially right on both sides, it is really a wretched undertaking, to persuade us that we must embark *our* angry and scornful passions on one side or the other.

I hope I have so far succeeded in shewing the natural but opposite tendencies of the two parties, as to make it almost unnecessary to enter into the question of which was legally in the right: for a question of that kind it is difficult to discuss without raising a contentious spirit. I may observe, however, that we can hardly help inferring, from Flamsteed's own shewing, that there was some strong ground of suspicion in the minds of those who urged the publication. Securities for Flamsteed's power and will to supply the press were scrupulously insisted on. Newton demanded a copy of the Catalogue before the work began: Flamsteed refused the Catalogue as being still incomplete; and also, as being his own: the matter was compromised, by the part already completed being put in Newton's hands *sealed up*. Various other negotiations of the same kind took place. Such demands would hardly have been made wantonly by Newton, who by his application to the Prince had been the means of procuring the printing of the Greenwich Observations. He wished to have security against Flamsteed's procrastination and fastidiousness. Flamsteed began by objecting to the printer as a bad workman. Still he was engaged; a work so inauspiciously begun was not likely to advance fast or well. There were accusations and counter-

accusations of stopping the press on each side: disputes arose about alterations introduced by the editor. Flamsteed then declined going on under direction of the Referees. The sealed Catalogue was broken open and put in the press under Halley's direction; and the whole of the proceedings became a train of anger, harshness, confusion, and delay, that did not end with Flamsteed's life.

As I have said, I shall not attempt to give sentence on the rights here in question; but I may observe, that the case on the side of Newton and his friends is very different from that which the Reviewer has collected, by confining himself to, and, what is more extraordinary, by adopting without modification, the indignant and querulous account given by poor Flamsteed, under the irritation of a fretful temper, hard work, old age, ill health, and constant quarrelling. It must be recollect that any assumption on the part of Flamsteed, that he might deal with the observations made in his official capacity of Astronomer Royal, as if they were his private property, could not be allowed by the guardians of the institution;—that Newton and the persons who acted with him, acted not as private persons, nor at the suggestion of their own caprice, but took measures for the publication, as the Visitors of the Observatory, bound by their duty to see the office made effective;—that *the sealed packet* being thus national property, the seal was declared to have been broken by the Queen's command;—and that when it appeared that Flamsteed's own *Historia Celestis* was likely to be completed, the Lords of the Treasury directed the copies of Halley's edition to be delivered into his hands, which was done.

As Flamsteed's statement of the case has been so fully brought before the public and so strongly urged, it is fitting that the statement of the other side should be at least attended to. It is found in the Preface to Halley's edition of the Catalogue, and with an abridgement of this statement I shall conclude; leaving it to the Reader to decide—whether the blame of intemperate virulence of feeling and irrational violence of conduct does not rest solely with Flamsteed;—whether Newton's philosophical and moral character do not come out from this examination blameless and admirable, as they have always been esteemed by thinking men;—and whether the Reviewer has not shewn extraordinary ignorance of that part of scientific history which he has pretended to elucidate, and unaccountable blindness and perverseness in his use even of the *ex parte* evidence which he had before him.

“Almost thirty years had elapsed during which Flamsteed possessed the title of Astronomer Royal, and nothing had proceeded from the magnificent Observatory with which he was entrusted. He appeared to have laboured only for himself or for a small number of friends; nothing was known, except that he was not idle, and that the registers of the Greenwich Observations were very voluminous.

“Prince George of Denmark referred it to Francis Roberts, Christopher Wren, Isaac Newton, David Gregory, and John Arbuthnot, to examine the manuscripts of Flamsteed, and to select that part which appeared to them worthy to see the light. This selection was made, and the Prince undertook the printing for the benefit of Astronomy and Navigation.

“ The Referees made, signed, and sealed an agreement with Mr Flamsteed for this impression. It was agreed that the work should consist of two parts; that there should be inserted in it the Observations made with the sextant previous to 1690; that the Catalogue should stand first; and that the second part should contain the Observations with the mural. Mr Flamsteed agreed to revise the proofs, and to supply the manuscript in time.

“ The work at first proceeded prosperously; but the printing was interrupted, first, because the Catalogue was extremely incomplete; and then by the death of the Prince of Denmark. The Queen however directed the undertaking to be continued, and gave the special charge of it to Dr Arbuthnot; and Mr Flamsteed, besides his constant occupation of observing the heavens, having his sight weakened by age, Edmund Halley was selected to expedite and complete the edition. Halley compared the Catalogue, which was the most important part, with the original observations; corrected a number of errors committed by the copyist or the calculator, and supplied numerous gaps; he made over again with care the calculations of the zodiacal stars; he determined from the observations of Flamsteed, the stars of six of the most northern constellations, which constitute nearly a fifth part of the Catalogue, and required immense calculations; he also added some southern constellations.

“ A selection was made of the observations of the planets, which were compared with those stars which came nearest to them in declination. All those in which there was reason to fear errors in the plane

of the instrument were rejected ; but all the observations of the Moon were printed."

The reader is then required to excuse the errors of the press, and some other slips "*of which Mr Flamsteed, better than any other person, knows the cause.*"

This is the edition of 1712, of which Flamsteed burnt 300 copies in 1714 ; making, as he indulged himself by writing in his Journal, "*a sacrifice of them to Heavenly Truth* ; as I should do of all the rest of my editor's pains of the like nature, if the Author of Truth should put them into my power." (p. 101.) He was then in his sixty-ninth year. His own edition was not finished at the time of his death in 1719 ; it did not appear till six years later.

TRINITY COLLEGE,
Dec. 21, 1835.

TO THE
EDITOR OF THE QUARTERLY REVIEW.

TRINITY COLLEGE, CAMBRIDGE,

February 3, 1836.

MY DEAR SIR,

I HAVE just seen No. 110 of the Review; and I perceive that the Reviewer of Mr Baily's account of Flamsteed, in No. 109, has done my remarks on his Article the honour of writing a Note respecting them, which you have inserted.

As I do not see in this Note any new arguments on the Reviewer's side of our controversy, I do not conceive that I have occasion to add much to what I have already said, for I presume your readers do not look for an answer to mere hard words. A few additional remarks will, I think, enable competent judges to decide between us.

I asserted, and assert, that Flamsteed never fully comprehended or accepted Newton's theory;—never understood the difference between the Newtonian theory of the *causes* of the celestial motions, and the *empirical laws of phenomena* which he himself called theories;—in short, the difference between a formula and an explanation—between the discovery of *what* occurred, and the discovery *why* it occurred

—between an observer and a philosopher. I quoted a letter which proved this; nor does the Reviewer venture to deny the clear inference which irresistibly follows from this quotation. But he takes refuge in “the whole tenour of the correspondence,” without quoting a single passage. To any one capable of understanding the distinction which I have pointed out, the whole tenour of the correspondence shews Flamsteed to have had no glimpse of this difference. For example, he says (Account of Flamsteed, &c., p. 211) of the theory, “*I call it mine*, because it consists of my solar and lunar tables corrected by myself, and shall own nothing of Mr Newton’s labours till he fairly owns what he has had from the Observatory;” and (p. 214) he says, that Newton “would needs question the observations when they agreed not with his theories, *or rather conceptions*.” The book is full of such expressions. The Edinburgh Reviewer, wiser than his brother, has pointed out this.

When my opponent has produced any one passage which shews that Flamsteed understood the difference between the nature of his own labours and those of Newton (which these passages and many others prove he did not understand), we shall be able to appreciate his claims to use language like that which he has applied to my opinions. Till then, such expressions as “audacious *dictum*,” and “we must beg our non-undergraduate public to consider,” must, I think, pass for bold words used to supply the lack of proofs.

I repeat also, that Flamsteed’s complaining that the English nation was robbed, because Newton’s theory of comets was confirmed by French observa-

tions, is another proof that Flamsteed did not understand what the nature, interest, or value of a true theory was.

With regard to the hard terms alledged by Flamsteed to have been used by Newton, I should, I think, have conveyed more exactly the impression which Flamsteed's angry statement leaves on calm consideration, by saying that it is probable that when Flamsteed had talked of the Royal Society as the robbers of his property, Newton did, in some way, employ the term "puppy;" but that it is certain that this was the hardest word which he was provoked to use; for it is abundantly clear that if any thing worse had been said, Flamsteed was not in a temper, or of a character, to abstain from recording it. The Reviewer's argument amounts to this:—that an angry man cannot exaggerate or misrepresent, because a clergyman ought not to lie. I do not think this will avail him.

On the subject of the sealed packet, I will put the issue in the form of a question. What does the Reviewer take to have been the *purpose* of depositing the observations in Newton's hands? My answer is simple. From Flamsteed's known irritability, it was thought necessary to require this deposit, in order to secure the publication, in case Flamsteed should refuse to proceed. The case provided for arrived: the remedy was applied. I want to hear of any *other* interpretation of the deposit.

The exclamatory way in which the Reviewer disposes of the account given by Arbuthnot of this step, appears to me rather tragical than logical. "The Queen's command. What a paltry, pitiful

subterfuge! The Queen's command! How often is the name of royalty thus abused!" The evidence that it had been abused in this case is, I believe, only Flamsteed's opinion—"This I am persuaded was false" (p. 294)—which I hold to be altogether insufficient, even if he had been an uninterested and reasonable person.

The Note quotes a passage of my Remarks, in which I had said that *I left it to the reader to decide* "whether the Reviewer had not shewn an extraordinary ignorance of that part of scientific history," &c. As I wrote with the wish of avoiding any thing offensive, I have once or twice since been disposed to regret that I had not left this decision to the reader, without *saying* that I had done so. I feel much less of this regret after reading the Reviewer's acknowledgement respecting the preface to the first edition of the Observations, that "he certainly is ignorant of this preface;" and after his speaking of it as a want of *candour* to call it Halley's, which no person at all acquainted with the history of astronomy needs to be informed. As to the statement made in this preface, I need not inform those who have read my Remarks, that I did not put it forward as unquestionable authority, but as the case on one side, in opposition to the ex parte statement made by the Reviewer on the other. There is, however, this material difference;—that this statement of Halley's was published to the world, and challenged contradiction; that adopted by the Reviewer is found in the moody soliloquies and querulous effusions of a weak man, which did not see the light till a hundred and thirty years later. As to Flamsteed's charges against

Halley's edition, I can hardly suppose that the Reviewer will carry any unprejudiced reader with him when he adopts them; though this proceeding is certainly in the general spirit of his treatment of the subject.

I did not argue the question of right in my Remarks; but I must now say that I am very far from assenting to the statements on this subject which have been published. The question of the kind of constraint which the nation has a right to exercise over the publication of the Astronomer Royal's Observations, I conceive to be a very difficult one: but Halley's statement that the Observatory had existed for thirty years and that nothing had been published, is a strong *prima facie* case; for it would be absurd to suppose that the Observer was at liberty to lock up his observations for ever. What would be the use of such an Observatory? or the meaning of its having Visitors? I must observe here that the Reviewer has, very unwarrantably, transformed the statement that nothing was *published*, into a charge that nothing was *done*. The complaint was, that though much was done, nobody but the Observer could profit by it.

I do not think it a reasonable infliction either on the reader or the writer, that a discussion of the character of one man should ramify into controversies on the merits of several others; and therefore I shall say as little as possible respecting Halley and Whiston. Halley, an eminent and vigorous philosopher, who devoted himself to science in the most liberal and useful manner during a long life, I hope to see vindicated, by some one acquainted with the history of those times, from

the aspersions which the childish spleen and gall of an irritated rival threw upon him, and which have been so strangely and precipitately adopted by men of the present day. I lament his or any one's errors; but when the Reviewer reminds us of the exclusion of Halley from the Savilian professorship on the ground of his want of religion, we may, perhaps, allow ourselves to hope that his subsequent election to the office implies that such unhappy opinions had been discarded. The charges of ignorance and immoral conduct are utterly at variance with all we know of him; and rest on nothing but Flamsteed's extravagant prejudices and passions servilely adopted by the Reviewer. The friend of Newton, the favoured servant of King William, Queen Anne, Queen Caroline, to whom the offer was made of being appointed preceptor to the Duke of Cumberland, was never by any other person accused of want of respectability: and the man whom Lalande termed the greatest of English astronomers, and whom the severe-judging Delambre calls one of the most eminent men of science that Europe has produced, can suffer little from Flamsteed's disparagement of his knowledge.

I hold Whiston's testimony to be of small value (not that *he himself* was a *worthless person*, as the Reviewer takes the liberty of misquoting me) from the extraordinary inconsistency, prejudice, and self-conceit, which I find in his Memoirs of himself. That he had some mathematical knowledge is little to the purpose; though, even in such subjects, I suppose the Reviewer is not prepared to admire the judgment which led him to recommend the scheme of finding the longitude by having ships moored all

over the surface of the ocean, each to fire a gun at midnight, so as to be heard and seen at any place.

The Reviewer states that Halley also kept his observations of the moon long unpublished, in order to have a chance of obtaining the reward for the longitude, and asks, "What does Mr Whewell think of private property now?" To which I answer, that I think of Halley's property as I think of Flamsteed's. Halley did publish, and with dispatch, his other observations. I have never either defended or blamed his holding back the lunar observations; but I may observe that the crisis which gave the peculiar importance to the publication of Flamsteed's was past; and I do not think Halley's motive at all reprehensible. In all such cases it is difficult to decide what constraint may be applied so as to produce publication. There may be a fault of procrastination and fastidiousness, which was Flamsteed's. The attempt to expedite publication in the manner which may be most advantageous to astronomy is meritorious; and this merit was Halley's, and Newton's. Whether in pursuit of this object they went beyond the limits which it is so difficult to define, I do not pronounce; but I am sure that Flamsteed was no judge of those limits; and his evidence is so far damaged by his circumstances and character, that it hardly helps us in deciding the point.

When you Reviewers condescend to controversy, you have an overwhelming advantage in being advocate and judge at the same time. I presume it is in a momentary usurpation of the latter capacity that my opponent calls my pamphlet "rash," "un-

worthy," &c. And when, moreover, to the circulation and authority of the Quarterly, you add the rapid reply of a monthly periodical, as in the present case, a poor pamphleteer has no chance of being heard in opposition to you. I shall therefore take the vehicle nearest at hand for this letter, and send it to the Cambridge paper; by which means it may, I hope, come to the knowledge of several of those who care most about the question.

Believe me, my dear Sir,

Yours very faithfully,

W. WHEWELL.

TO THE

EDITOR OF THE CAMBRIDGE CHRONICLE.

SIR,

I SHALL be much obliged by your publishing this letter as a postscript to that addressed to the Editor of the Quarterly Review, which you did me the favour of inserting in last week's Chronicle.

Some of my friends, feeling that strong interest in the fair fame of Newton which those cannot fail to feel who love to contemplate the union of intellectual and moral excellence, have expressed regret at my not having answered the charge that Newton neglected to acknowledge his obligation to Flamsteed for the observations by which the numerical elements

of the Lunar Theory were determined ; and that in the second edition of the *Principia* he erased the acknowledgement he had made in the first. I had passed over this point, as not bearing materially on the dispute respecting the publication of Flamsteed's observations, which appears to have attracted the largest share of the notice of the public ; and with a view of abridging, as much as justice would permit, this unprofitable discussion of the errors and weaknesses of those whom we have been accustomed to admire : but a few words on the subject just mentioned may serve to show how much of mistake there is in such statements.

That the Newtonian Lunar Theory was published the second time without any acknowledgement of what it owed to Flamsteed, is not true. Newton's "Theory of the Moon," on its first appearance after the use of Flamsteed's observations, and on the only occasion (so far as I know) when it was published with that title, was inserted in David Gregory's *Astronomiae Physicæ et Geometricæ Elementa*, printed in 1702. It is there stated (p. 332) that the illustrious author had made the calculations agree very nearly with the phenomena, "as he had proved by very many places of the Moon observed by the celebrated Mr Flamsteed." And the elements of the theory are there by Newton referred to Greenwich. With this book, Flamsteed was on various accounts much discontented. One great reason was, that Gregory had said, "The most solid walls, and even rocks and mountains, are not absolutely steady;" "this," says Flamsteed, "is a fling at my wall-arc." (Flamsteed, p. 204.) But I do not see that he here complains of any omission

of his name in the Lunar Theory. Newton had previously communicated his theory to Flamsteed, in the shape in which the observer could understand and use it (Flamsteed, p. 72); and though Flamsteed speaks contemptuously and disparagingly of it, he employed it in constructing Lunar Tables, which he called a Theory. It is of this that he says, a little before the publication of Gregory's work, (p. 211) "I call it mine, and shall own nothing of Mr Newton's labours, till he fairly owns what he has had from the Observatory." The obligations of the theory of universal gravitation to Flamsteed, were of the same nature as its obligations to Tycho Brahe, who believed that the sun went round the earth. The observations were highly useful; but it would have been an absurd perversion of the truth to have called the observer one of the authors of the theory. Yet it is probable that nothing less than this, and probably not this, would have satisfied the discontented and morbid mind of Flamsteed. What was stated in Gregory's book was just; and I do not see what more could have been briefly said.

By the time of the publication of the second edition of the *Principia* in 1713 (the year before the *sacrifice to Heavenly Truth*), the impossibility of noticing Flamsteed in any manner which would not disgust and irritate him, must have been very clear. Newton appears therefore only to have acted with common prudence and forbearance in avoiding such notice as much as possible. Flamsteed is not quoted as authority for the Lunar Theory, of which he rejected a great part. (See Account of Flamsteed, pp. 304, 305, 309.) His observations of the Comet are quoted as the best. In several other

points, as the observations of the satellites of Jupiter, Newton refers to published observations of other astronomers, instead of the private communications of Flamsteed. It was proper to reason upon published rather than upon unpublished observations; and the terms on which Flamsteed had put himself with Newton were probably felt by the great philosopher to be such as rendered it undesirable to make use of the private letters of his perverse correspondent.

So far as the published letters of Flamsteed prove anything, they shew, that not only he did not feel himself injured by not being mentioned in those parts of the second edition of the *Principia* which refer to the Moon, but that he entertained such an opinion of the work as would have made him angry at being so introduced. Thus, soon after the publication, he says, (p. 305) "I think his new *Principia* worse than the old." And (p. 309) he writes to his friend Abraham Sharp, "I have determined to lay these crotches of Sir I. Newton wholly aside; and I think if you purchase not the new edition of his book [of which the price was 18s.] you will be at least 17s. a saver by it; for I know not whether all the alterations and additions be worth 12d."

So much for the wrong done to Flamsteed by not being sufficiently mentioned in the second edition of the *Principia*. I have been told also that I ought to have noticed more particularly some of the extravagant expressions of assumed authority and intemperate accusation which occur in the Note in the Quarterly Review: but as these can affect only the character of the anonymous Reviewer, I do not see how it can be worth while to make them the subject of remark.

I will again *leave it to the reader to decide*, after looking at the passages I have just produced, whether the writer of the Note, in appealing to "the whole tenour of the book," as proving that Flamsteed comprehended and accepted Newton's Theory, was not asserting at random, and taking the chance of the impression he might produce, without having read the work which was under his review, or understanding the question on which he undertook to pronounce.

I suppose that if the vilifier of Newton has nothing to support him but rhetoric of this kind, the admirers of that great man will not feel any permanent quietude; and my sole object will be answered.

I am, Sir,

Your very obedient servant,

W. WHEWELL.

TRINITY COLLEGE,
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I shall conclude, leaving it to the reader to decide, whether the blame of intemperate virulence of feeling, and irrational violence of conduct, does not rest solely with Flamsteed; whether Newton's philosophical and moral character do not come out from this examination blameless, and admirable as they have always

been esteemed by thinking men; and whether the Reviewer has not shown extraordinary ignorance of that part of scientific history which he has attempted to elucidate, and unaccountable blindness and perverseness in his use even of the *ex parte* evidence which he had before him.

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